

Ever Since Darwin

Reflections
in Natural
History

Stephen Jay Gould



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IN 1758, LINNAEUS faced the difficult decision of how to classify his own species in the definitive edition of his *Systema Naturae*. Would he simply rank *Homo sapiens* among the other animals or would he create for us a separate status? Linnaeus compromised. He placed us within his classification (close to monkeys and bats), but set us apart by his description. He defined our relatives by the mundane, distinguishing characters of size, shape, and number of fingers and toes. For *Homo sapiens*, he wrote only the Socratic injunction: *nosce te ipsum*—"know thyself."

For Linnaeus, *Homo sapiens* was both special and not special. Unfortunately, this eminently sensible resolution has been polarized and utterly distorted by most later commentators. Special and not special have come to mean nonbiological and biological, or nurture and nature. These later polarizations are nonsensical. Humans are animals and everything we do lies within our biological potential. Nothing arouses this ardent (although currently displaced) New Yorker to greater anger than the claims of some self-styled "ecoactivists" that large cities are the "unnatural" harbingers of our impending destruction. But—and here comes the biggest *but* I can muster—the statement that humans are animals does not imply that our specific patterns of behavior and social arrangements are in any way directly determined by our genes. *Potentiality* and *determination* are different concepts.

The intense discussion aroused by E. O. Wilson's *Sociobi-*

ology (Harvard University Press, 1975) has led me to take up this subject. Wilson's book has been greeted by a chorus of praise and publicity. I, however, find myself among the smaller group of its detractors. Most of *Sociobiology* wins from me the same high praise almost universally accorded it. For a lucid account of evolutionary principles and an indefatigably thorough discussion of social behavior among all groups of animals, *Sociobiology* will be the primary document for years to come. But Wilson's last chapter, "From Sociobiology to Sociology," leaves me very unhappy indeed. After twenty-six chapters of careful documentation for the nonhuman animals, Wilson concludes with an extended speculation on the genetic basis of supposedly universal patterns in human behavior. Unfortunately, since this chapter is his statement on the subject closest to all our hearts, it has also attracted more than 80 percent of all the commentary in the popular press.

We who have criticized this last chapter have been accused of denying altogether the relevance of biology to human behavior, of reviving an ancient superstition by placing ourselves outside the rest of "the creation." Are we pure "nurturists?" Do we permit a political vision of human perfectibility to blind us to evident constraints imposed by our biological nature? The answer to both statements is no. The issue is not universal biology vs. human uniqueness, but biological potentiality vs. biological determinism.

Replying to a critic of his article in the *New York Times Magazine* (October 12, 1975), Wilson wrote:

There is no doubt that the patterns of human social behavior, including altruistic behavior, are under genetic control, in the sense that they represent a restricted subset of possible patterns that are very different from the patterns of termites, chimpanzees and other animal species.

If this is all that Wilson means by genetic control, then we can scarcely disagree. Surely we do not do all the things that other animals do, and just as surely, the range of our potential behavior is circumscribed by our biology. We would lead very different social lives if we photosynthesized (no agricul-

ture, gathering, or hunting—the major determinants of our social evolution) or had life cycles like those of the gall midges discussed in essay 10. (When feeding on an uncrowded mushroom, these insects reproduce in the larval or pupal stage. The young grow within the mother's body, devour her from inside, and emerge from her depleted external shell ready to feed, grow the next generation, and make the supreme sacrifice.)

But Wilson makes much stronger claims. Chapter 27 is not a statement about the range of potential human behaviors or even an argument for the restriction of that range from a much larger total domain among all animals. It is, primarily, an extended speculation on the existence of genes for specific and variable traits in human behavior—including spite, aggression, xenophobia, conformity, homosexuality, and the characteristic behavioral differences between men and women in Western society. Of course, Wilson does not deny the role of nongenetic learning in human behavior; he even states at one point that "genes have given away most of their sovereignty." But, he quickly adds, genes "maintain a certain amount of influence in at least the behavioral qualities that underlie variations between cultures." And the next paragraph calls for a "discipline of anthropological genetics."

Biological determinism is the primary theme in Wilson's discussion of human behavior; chapter 27 makes no sense in any other context. Wilson's primary aim, as I read him, is to suggest that Darwinian theory might reformulate the human sciences just as it previously transformed so many other biological disciplines. But Darwinian processes can not operate without genes to select. Unless the "interesting" properties of human behavior are under specific genetic control, sociology need fear no invasion of its turf. By interesting, I refer to the subjects sociologists and anthropologists fight about most often—aggression, social stratification, and differences in behavior between men and women. If genes only specify that we are large enough to live in a world of gravitational forces, need to rest our bodies by sleeping, and do not photosynthesize, then the realm of genetic determinism will be relatively uninspiring.

What is the direct evidence for genetic control of specific human social behavior? At the moment, the answer is none whatever. (It would not be impossible, in theory, to gain such evidence by standard, controlled experiments in breeding, but we do not raise people in *Drosophila* bottles, establish pure lines, or control environments for invariant nurturing.) Sociobiologists must therefore advance indirect arguments based on plausibility. Wilson uses three major strategies: universality, continuity, and adaptiveness.

1. Universality: If certain behaviors are invariably found in our closest primate relatives and among humans themselves, a circumstantial case for common, inherited genetic control may be advanced. Chapter 27 abounds with statements about supposed human universals. For example, "Human beings are absurdly easy to indoctrinate—they *seek* it." Or, "Men would rather believe than know." I can only say that my own experience does not correspond with Wilson's.

When Wilson must acknowledge diversity, he often dismisses the uncomfortable "exceptions" as temporary and unimportant aberrations. Since Wilson believes that repeated, often genocidal warfare has shaped our genetic destiny, the existence of nonaggressive peoples is embarrassing. But he writes: "It is to be expected that some isolated cultures will escape the process for generations at a time, in effect reverting temporarily to what ethnographers classify as a pacific state."

In any case, even if we can compile a list of behavioral traits shared by humans and our closest primate relatives, this does not make a good case for common genetic control. Similar results need not imply similar causes; in fact, evolutionists are so keenly aware of this problem that they have developed a terminology to express it. Similar features due to common genetic ancestry are "homologous"; similarities due to common function, but with different evolutionary histories, are "analogous" (the wings of birds and insects, for example—the common ancestor of both groups lacked wings). I will argue below that a basic feature of human biology supports the idea that many behavioral similarities between humans and other primates are analogous, and that they have no direct genetic specification in humans.

2. Continuity: Wilson claims, with ample justice in my opinion, that the Darwinian explanation of altruism in W. D. Hamilton's 1964 theory of "kin selection" forms the basis for an evolutionary theory of animal societies. Altruistic acts are the cement of stable societies, yet they seem to defy a Darwinian explanation. On Darwinian principles, all individuals are selected to maximize their own genetic contribution to future generations. How, then, can they willingly sacrifice or endanger themselves by performing altruistic acts to benefit others?

The resolution is charmingly simple in concept, although complex in technical detail. By benefiting relatives, altruistic acts preserve an altruist's genes even if the altruist himself will not be the one to perpetuate them. For example, in most sexually reproducing organisms, an individual shares (on average) one-half the genes of his sibs and one-eighth the genes of his first cousins. Hence, if faced with a choice of saving oneself alone or sacrificing oneself to save more than two sibs or more than eight first cousins, the Darwinian calculus favors altruistic sacrifice; for in so doing, an altruist actually increases his own genetic representation in future generations.

Natural selection will favor the preservation of such self-serving altruist genes. But what of altruistic acts toward non-relatives? Here sociobiologists must invoke a related concept of "reciprocal altruism" to preserve a genetic explanation. The altruistic act entails some danger and no immediate benefit, but if it inspires a reciprocal act by the current beneficiary at some future time, it may pay off in the long run: a genetic incarnation of the age-old adage: you scratch my back and I'll scratch yours (even if we're not related).

The argument from continuity then proceeds. Altruistic acts in other animal societies can be plausibly explained as examples of Darwinian kin selection. Humans perform altruistic acts and these are likely to have a similarly direct genetic basis. But again, similarity of result does not imply identity of cause (see below for an alternate explanation based on biological potentiality rather than biological determinism).

3. Adaptiveness: Adaptation is the hallmark of Darwinian processes. Natural selection operates continuously and re-

lently to fit organisms to their environments. Disadvantageous social structures, like poorly designed morphological structures, will not survive for long.

Human social practices are clearly adaptive. Marvin Harris has delighted in demonstrating the logic and sensibility of those social practices in other cultures that seem most bizarre to smug Westerners (*Cows, Pigs, Wars, and Witches*. Random House, 1974). Human social behavior is riddled with altruism; it is also clearly adaptive. Is this not a *prima facie* argument for direct genetic control? My answer is definitely “no,” and I can best illustrate my claim by reporting an argument I recently had with an eminent anthropologist.

My colleague insisted that the classic story of Eskimos on ice floes provides adequate proof for the existence of specific altruist genes maintained by kin selection. Apparently, among some Eskimo peoples, social units are arranged as family groups. If food resources dwindle and the family must move to survive, aged grandparents willingly remain behind (to die) rather than endanger the survival of their entire family by slowing an arduous and dangerous migration. Family groups with no altruist genes have succumbed to natural selection as migrations hindered by the old and sick lead to the death of entire families. Grandparents with altruist genes increase their own fitness by their sacrifice, for they enhance the survival of close relatives sharing their genes.

The explanation by my colleague is plausible, to be sure, but scarcely conclusive since an eminently simple, nongenetic explanation also exists: there are no altruist genes at all, in fact, no important genetic differences among Eskimo families whatsoever. The sacrifice of grandparents is an adaptive, but nongenetic, cultural trait. Families with no tradition for sacrifice do not survive for many generations. In other families, sacrifice is celebrated in song and story; aged grandparents who stay behind become the greatest heroes of the clan. Children are socialized from their earliest memories to the glory and honor of such sacrifice.

I cannot prove my scenario, any more than my colleague can demonstrate his. But in the current context of no evidence, they are at least equally plausible. Likewise, reciprocal

altruism undeniably exists in human societies, but this provides no evidence whatever for its genetic basis. As Benjamin Franklin said: “We must all hang together, or assuredly we shall all hang separately.” Functioning societies may require reciprocal altruism. But these acts need not be coded into our consciousness by genes; they may be inculcated equally well by learning.

I return, then, to Linnaeus’s compromise—we are both ordinary and special. The central feature of our biological uniqueness also provides the major reason for doubting that our behaviors are directly coded by specific genes. That feature is, of course, our large brain. Size itself is a major determinant of the function and structure of any object. The large and the small cannot work in the same way (see section 6). The study of changes that accompany increasing size is called “allometry.” Best known are the structural changes that compensate for decreasing surface/volume ratios of large creatures—relatively thick legs and convoluted internal surfaces (lungs, and villi of the small intestine, for example). But markedly increased brain size in human evolution may have had the most profound allometric consequences of all—for it added enough neural connections to convert an inflexible and rather rigidly programmed device into a labile organ, endowed with sufficient logic and memory to substitute non-programmed learning for direct specification as the ground of social behavior. Flexibility may well be the most important determinant of human consciousness (see essay 7); the direct programming of behavior has probably become inadaptable.

Why imagine that specific genes for aggression, dominance, or spite have any importance when we know that the brain’s enormous flexibility permits us to be aggressive or peaceful, dominant or submissive, spiteful or generous? Violence, sexism, and general nastiness *are* biological since they represent one subset of a possible range of behaviors. But peacefulness, equality, and kindness are just as biological—and we may see their influence increase if we can create social structures that permit them to flourish. Thus, my criticism of Wilson does not invoke a nonbiological “environmentalism”; it merely pits the concept of biological potentiality—a

brain capable of the full range of human behaviors and rigidly predisposed toward none—against the idea of biological determinism—specific genes for specific behavioral traits.

But why is this academic issue so delicate and explosive? There is no hard evidence for either position, and what difference does it make, for example, whether we conform because conformer genes have been selected or because our general genetic makeup permits conformity as one strategy among many?

The protracted and intense debate surrounding biological determinism has arisen as a function of its social and political message. As I argue in the preceding set of essays, biological determinism has always been used to defend existing social arrangements as biologically inevitable—from “for ye have the poor always with you” to nineteenth-century imperialism to modern sexism. Why else would a set of ideas so devoid of factual support gain such a consistently good press from established media throughout the centuries? This usage is quite out of the control of individual scientists who propose deterministic theories for a host of reasons, often benevolent.

I make no attribution of motive in Wilson’s or anyone else’s case. Neither do I reject determinism because I dislike its political usage. Scientific truth, as we understand it, must be our primary criterion. We live with several unpleasant biological truths, death being the most undeniable and ineluctable. If genetic determinism is true, we will learn to live with it as well. But I reiterate my statement that no evidence exists to support it, that the crude versions of past centuries have been conclusively disproved, and that its continued popularity is a function of social prejudice among those who benefit most from the status quo.

But let us not saddle *Sociobiology* with the sins of past determinists. What have been its direct results in the first flush of its excellent publicity? At best, we see the beginnings of a line of social research that promises only absurdity by its refusal to consider immediate nongenetic factors. The January 30, 1976, issue of *Science* (America’s leading technical journal for scientists) contains an article on panhandling that I would have accepted as satire if it had appeared verbatim in the

National Lampoon. The authors dispatched “panhandlers” to request dimes from various “targets.” Results are discussed only in the context of kin selection, reciprocal altruism, and the food-sharing habits of chimps and baboons—nothing on current urban realities in America. As one major conclusion, they find that male panhandlers are “far more successful approaching a single female or a pair of females than a male and female together; they were particularly unsuccessful when approaching a single male or two males together.” But not a word about urban fear or the politics of sex—just some statements about chimps and the genetics of altruism (although they finally admit that reciprocal altruism probably does not apply—after all, they argue, what future benefit can one expect from a panhandler).

In the first negative comment on *Sociobiology*, economist Paul Samuelson (*Newsweek*, July 7, 1975) urged sociobiologists to tread softly in the zones of race and sex. I see no evidence that his advice is being heeded. In his *New York Times Magazine* article of October 12, 1975, Wilson writes:

In hunter-gatherer societies, men hunt and women stay at home. This strong bias persists in *most* [my emphasis] agricultural and industrial societies and, on that ground alone, appears to have a genetic origin. . . . My own guess is that the genetic bias is intense enough to cause a substantial division of labor even in the most free and most egalitarian of future societies. . . . Even with identical education and equal access to all professions, men are likely to continue to play a disproportionate role in political life, business and science.

We are both similar to and different from other animals. In different cultural contexts, emphasis upon one side or the other of this fundamental truth plays a useful social role. In Darwin’s day, an assertion of our similarity broke through centuries of harmful superstition. Now we may need to emphasize our difference as flexible animals with a vast range of potential behavior. Our biological nature does not stand in the way of social reform. We are, as Simone de Beauvoir said, “l’être dont l’être est de n’être pas”—the being whose essence lies in having no essence.

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